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The Effectiveness of Hiring Credits

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This article analyses the effectiveness of hiring credits. Using comprehensive administrative data, we show that the French hiring credit, implemented during the Great Recession, had significant positive employment effects and no effects on wages. Relying on the quasi-experimental variation in labour cost triggered by the hiring credit, we estimate a structural search and matching model. Simulations of counterfactual policies show that the effectiveness of the hiring credit relied to a large extent on three features: it was non-anticipated, temporary and targeted at jobs with rigid wages. We estimate that the cost per job created by permanent hiring credits, either countercyclical or time-invariant, in an environment with flexible wages would have been much higher.

Key words: Hiring credit, Labour demand, Search and matching model.

JEL Codes: C31, C93, J6

1. INTRODUCTION

Hiring credits have been used in the U.S. and in a number of European countries to counteract the employment effects of the 2008–9 recession.[1] Despite this wide use, many economists think that hiring credits are probably useless during recessions, when aggregate demand is insufficient relative to labour and other resources available in the economy.[2] In fact, there is very little empirical evidence about the effects of hiring credits. Evidence on federal programmes in the U.S. dates back to the 80s (Perloff and Wachter, 1979; Bishop, 1981), and the only recent evidence concerns hiring credits implemented at the U.S. states level (Neumark and Grijalva, 2017). We seize the opportunity of the natural experiment induced by the 2009 French hiring credit to highlight the

1. See OECD (2010) for a detailed presentation of hiring credit measures in 2009.
2. For instance, Becker (2010), Posner (2010) and Gali (2013).

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effectiveness of such policies. Reduced-form estimates of the effects of the French programme relying on comprehensive administrative data show that the hiring credit has had a significant impact on employment. Then, we use quasi-experimental variations induced by the programme to estimate key structural parameters of a search and matching model. Simulations of this model show that the cost per job created is very sensitive to the type of hiring credit—temporary versus permanent, countercyclical versus time-invariant,[3] generalized to all hires versus targeted at a small subset of hires—and to the economic environment—rigid versus flexible wages.

The French hiring credit, announced on 4 December 2008, relieved firms from social contributions on new hires until 31 December 2009. The programme was arbitrarily restricted, for budgetary reasons, to firms with fewer than 10 employees, and to low-wage workers. We show that these restrictions and other features of the programme ensure that its implementation can be considered as a natural experiment. Moreover, only a small fraction of hires were actually eligible for the hiring credits so that the programme did not trigger spillover effects.

Our evaluation of the French hiring credit relies on two identification strategies. The difference-in-differences strategy compares the evolution of small firms (between 6 and 10 employees) and medium-size firms (between 10 and 14 employees) from November 2008, just before the program's inception, until November 2009.[4] The IV strategy compares employment pool × sector cells with high and low shares of subsidized hires. We use the share of low-wage workers from 2006 to 2008 as an instrument for the share of subsidized hires. Both strategies yield converging results. The French hiring credit significantly increased by 0.8 percentage point the growth rate of targeted firms. Moreover, the employment effects are concentrated as expected on eligible jobs, *i.e.* low-wage jobs. The impact of the hiring credit emerged quickly: hires and employment began to rise three months after the introduction of the credit. The evolution of hours worked is similar to that of employment, meaning that firms did not substitute hours of new workers benefiting from the hiring credit for those of incumbent employees. We find no increase in wages associated with the hiring credit, and firms did not increase layoffs to hire workers at lower cost. Year placebo tests, and robustness analysis varying the firms' size bandwidth selecting the estimation sample, confirm our results. Comparing ineligible medium-sized firms in labour markets with a high or low fraction of subsidized hires, we show that the hiring credit did not trigger equilibrium effects, either through wage adjustments or effects on labour market tightness.

Building on these reduced-form analyses, we use quasi-experimental variations in labour cost induced by the program to estimate a structural search and matching model. We show that the variations in the coverage of the hiring credit and in the tightness across local labour markets allow us to identify two key parameters: the elasticity of the marginal revenue (with respect to labour) and the vacancy posting cost. The time-variation in tightness and job finding rates within local labour markets before the 2009 French hiring credit allow us to identify a third key parameter, *i.e.* the elasticity of the matching function with respect to the number of job-seekers, as in Borowczyk-Martins *et al.* (2013).

Introducing directed search with wage posting into the model, in line with Moen (1997) we show, in the spirit of the sufficient statistic approach (Chetty, 2009), that the three structural parameters estimated above are sufficient to define the cost per job created by hiring credits in

3. By definition, hiring credits provide subsidies to new jobs for a limited period of time at the beginning of the job spell. Temporary hiring credits are one-off schemes that provide these subsidies during specific periods, whereas permanent hiring credits provide them permanently. Permanent hiring credits can be either time-invariant or countercyclical, *i.e.* provided in slowdowns only. Neumark and Grijalva (2017) report that 99 of the 147 hiring credits recorded in the United States over the period 1970–2012 are permanent.

4. Consistent with the program, we split firms according to their size computed from November 2007 to November 2008, before the program was announced.

different cases: exogenous versus endogenous wage, temporary versus permanent hiring credit (either countercyclical or time-invariant),[5] hiring credit generalized to all firms versus hiring credit targeted at a small subset of firms (sufficiently small to have no impact on the labour market tightness). Using our previous estimates of the structural parameters, we compute the cost per job created by these counterfactual policies. In the baseline scenario, which corresponds to the 2009 French hiring credit, the *gross* cost per job created is around one fourth of the average annual wage. To compute the cost per job created *net* of savings on social benefits, we exploit a survey that provides information about the characteristics of the beneficiaries of the hiring credit. It turns out that the 2009 hiring credit has been very effective, since the net cost per job created is about zero.

Nevertheless, our simulations suggest that the effectiveness of hiring credits is contingent on particular circumstances. In line with Kitao *et al.* (2010) and Kaas and Kircher (2015), we find that the (one-off, non-expected) temporary nature of hiring credits plays a key role: permanent hiring credits, even targeted at a small subset of firms, create jobs at a cost multiplied by a factor of four compared with one-off non-anticipated temporary credits available for new hires during one year.[6] Hiring credits generalized to all firms would have featured only a slightly higher gross cost per job created than a similar hiring credit targeted at a small subset of firms, as long as wages are exogenous. This result, obtained in a context of high unemployment rates, means that congestion effects induced by the hiring credit are too small to induce significant increases in recruitment costs. When wages are flexible, the cost per job created by temporary hiring credits generalized to all firms is only slightly higher than the cost per job created by temporary hiring credits targeted at a small subset of firms, because one-off temporary increases in labour market tightness induced by temporary hiring credits have little impact on the expected gains of unemployed workers, and therefore on wages. However, permanent economy-wide hiring credits induce permanent increases in labour market tightness, and thus have a stronger impact on the expected gains of unemployed workers and then on wages. We find that the reaction of wages multiplies by about 2 the cost per job created by generalized and permanent hiring credits. This casts doubt about the effectiveness of permanent hiring credits, which are frequent (OECD, 2010; Neumark and Grijalva, 2017).

Our article contributes to the empirical debate on the effectiveness of hiring credits. It is related to Neumark and Grijalva (2017) who analyse state hiring credits in the U.S.[7] Using a difference-in-differences strategy across U.S. states, they point to moderate positive employment effects of credits targeting the unemployed during recessions. To our knowledge, our article is the first empirical evaluation of a temporary hiring credit relying on comprehensive firm-level administrative data. The richness of the data and the quasi-experimental situation induced by the French hiring credit allow us to evaluate the impact of the hiring credit, with proper identification strategies, on a wide range of outcomes not available in previous studies. Moreover, our article is also the first empirical evaluation of a temporary hiring credit in Europe. European empirical evidence mostly concerns the effects of permanent payroll tax reductions. As both hires and

5. In this article, the cost per job created by a temporary hiring credit, set out for one year, corresponds to the monthly cost necessary to create one supplementary job at the one-year time horizon, whereas the cost per job created by a permanent hiring credit, set out without any foreseen time limit, is the monthly cost necessary to create one supplementary job permanently, *i.e.* on an infinite time horizon.

6. Note that permanent hiring credits are different from wage subsidies, as, for a given worker, the credit vanishes after a certain tenure in the firm—one year in our simulations.

7. Our work is also related to the evaluations of the New Job Tax Credit (NJTC) implemented in the U.S. during the 70s by Perloff and Wachter (1979) and Bishop (1981). Both studies find positive effects of the program, but their analyses suffer from the economy-wide implementation of the NJTC, which makes it difficult to define a proper counterfactual control group.

incumbents are eligible for these payroll tax reductions, they may imply large deadweight losses, and their estimated effects only partially inform us about the effects of hiring credits.[8] In Europe, hiring subsidies may also be part of broader strategies to activate the unemployed. They are frequently coupled with job search assistance programs, which makes it difficult to distinguish their impact, as in Blundell *et al.* (2004).[9]

We also contribute to the literature which builds bridges between quasi-experimental or experimental data and structural estimation, *i.e.* Attanasio *et al.* (2012), Ferrall (2012), Gautier *et al.* (2012), Galiani *et al.* (2015), Lise *et al.* (2015), Todd and Wolpin (2006) and Wise (1985). Our approach features both internal validity and external validity. The source of the identification of the key structural parameters is quasi-experimental and makes use of a well-defined policy shock. Thus we gain internal validity. Then simulations of the underlying economic model enable us to discuss the external validity of our reduced-form results. This framework is useful to quantify congestion externalities in search and matching models (Beaudry *et al.*, 2012, 2014; Gautier *et al.*, 2012; Crépon *et al.*, 2013; Lalive *et al.*, 2013). It is closely related to Beaudry *et al.* (2014) who show that the wage elasticity of employment is larger in absolute value at the industry-city level than at the city-level. They argue that the effects of wage shocks at the city-level are damped by congestion externalities induced by the reaction of the city-level labour market tightness. We also find that congestion externalities play a very important role through wages. Congestion externalities exert an upward pressure on wages that significantly reduces the employment effects of economy-wide hiring credits compared with hiring credits targeted at a small subset of firms.

The article is organized as follows. Section 2 describes the hiring credit scheme (*zéro charges*) implemented in France in 2009. Section 3 presents the data, descriptive statistics and the empirical strategy of the reduced-form approaches. The difference-in-differences estimates are presented in Section 4. The results of the IV estimation are presented in Section 5. Section 6 shows that the French program did not trigger equilibrium effects. Section 7 proceeds to the structural estimation of the search and matching model and evaluates the cost per job created by hiring credits in different environments. The last section concludes.

2. INSTITUTIONAL BACKGROUND

The *zéro charges* (zero contributions) measure was announced by the French President on 4 December 2008. According to the original announcement, any hire (or temporary contract renewal) of a low-wage worker in a firm with fewer than 10 employees occurring from the date of the announcement until 31 December 2009 could benefit during the same year from an employer social contribution relief.[10] The relief is maximal for workers with an hourly remuneration at the minimum wage level (8.82 euros in 2009). With *zéro charges*, employers do not pay any social contribution at the minimum wage level. The relief then decreases as the hourly wage level rises up to 1.6 times the minimum wage. Figure 1 shows that the hiring credit reduces the labour cost by 12% for a full-time worker paid at the minimum wage. The maximum amount of the hiring credit over 12 months represents 2,400 euros. When the wage is 30% above the minimum wage, the subsidy rate represents only 4% of the labour cost.

8. Indeed, Goos and Konings (2007), Huttunen *et al.* (2013), Bennmarker *et al.* (2009), Egebark and Kaunitz (2013) and Skedinger (2014) find rather small employment effects of permanent payroll tax reduction.

9. A notable attempt to distinguish the relative effectiveness of the different components of activation strategies is Sianesi (2008). For Sweden in the 1990s, she finds that entering a temporary job subsidy program rather than searching further in open unemployment increased employment rates soon after the program ended.

10. The new relief is in addition to the existing general social contribution reduction on low wages called the *Fillon reduction*, which has prevailed since the 1990s and concerns all firms in the private sector.

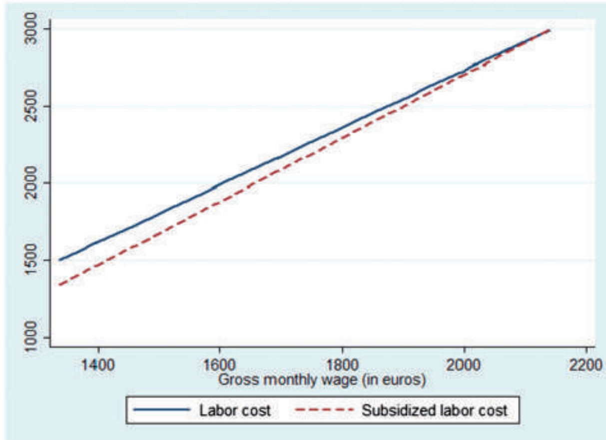

FIGURE 1

The hiring credit schedule.

Notes: The horizontal axis reports the monthly wage (in euros) net of employer social contributions of a full time worker (in 2009, the monthly minimum wage was 1,338 euros in gross terms, *i.e.* including employees' social contributions). The vertical axis reports the monthly labour cost. The continuous line displays the labour cost without the hiring credit. The dashed line shows the labour cost with the hiring credit.

Before the first announcement, the policy was not anticipated because it was kept secret as part of a stimulus package to be disclosed all at once.[11] This is illustrated by Figure 2 which shows that Google searches for the item "hiring subsidy" (*aide embauche*) started to increase in December 2008, once the announcement for the program was made. There is no Google search for the item *zéro charges* before early 2009.

The practical details of the hiring credit were rapidly set out in a decree published on 20 December 2008. To start with, only firms and associations belonging to the private sector could get the hiring credit. Firms and associations had to request the *zéro charges* relief for each hire separately, filling out a one-page form and attaching the labour contract. The claim had to be sent to the French Public Employment Service (Pôle emploi) which reimbursed for the social contributions payments on eligible hires at the end of each quarter.

Second, to be sponsored, hires had to be on contracts lasting at least one month, and not otherwise sponsored by other targeted special measures, such as even more generous and pre-existing subsidies for some disadvantaged groups (*e.g.* the long-term unemployed) or apprentices; household jobs were also excluded on the ground of their specific and pre-existing subsidies. The hiring credit was not restricted to firms with net employment growth, and it was not limited to the hiring of the long-term unemployed or any other disadvantaged groups.

Third, only entities with fewer than 10 full-time equivalent employees[12] on average between January and November 2008 could apply. Hence, the period used to define the size criteria ends

11. For instance the newspaper *Les Echos,* describes in an article entitled "Le gouvernement envisage d'accélérer ses paiements et remboursements aux entreprises", published on 27 November 2008, all potential measures that the President Sarkozy was supposed to announce the following week at the Press conference of 4 December 2008. The hiring credit is not mentioned in this article. On 4 December 2008, the article entitled "Sarkozy dévoile un plan de 26 milliards d'euros pour relancer l'économie", summarizing the contents of the press conference, does mention the hiring credit.

12. The size criteria are very precise and follow the usual rules set in the labour code (see cerfa n° 13838-01). Only ordinary employees are kept in the computation of the size (thus excluding apprentices and temporary agency employees and those hired as part of a labour market program). The size is computed as the average of the end-of-month number of employees from January to November 2008. Fixed-term workers contribute *pro rata temporis* their number of days

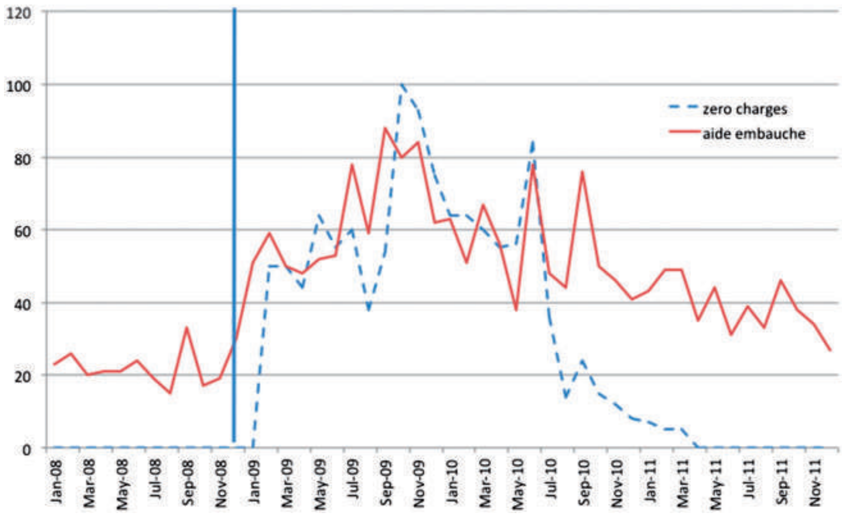

FIGURE 2

Results of Google search for the policy name.

Notes: The vertical axis reports the monthly number of searches for one term relative to the highest point on the figure; "aide embauche" means hiring subsidy. Variations in spellings (*e.g.* "zéro charges", "zero charge") yield similar patterns to "zero charges". The vertical line indicates the date at which the hiring credit was introduced. *Source*: Google Trends website.

just before the announcement of the policy, on 4 December 2008. A growing firm reaching 10 or more employees over the year 2009 could still continue to receive subsidies and apply for new hires until the end of 2009. This meant that the size criteria could not be manipulated by firms wishing to benefit from the hiring credit.

Fourth, applying firms must not have fired any workers for economic reasons on the same job over the six months preceding the hiring date, nor must they have previously fired this particular worker over the same period from any other job, and they must have paid all their social contributions in the past.

On 16 November 2009, the policy was extended to hires occurring up to 30 June 2010.[13] On this occasion, the duration of the hiring credit was extended for up to 12 months from the hiring date, instead of the cutoff date of 31 December 2009 for the initial scheme. This new rule was also applicable to hires made in 2009 before the announcement of the extension, and which already benefited from *zéro charges*. Firms below the average of 10 full-time equivalent employees from January 2009 to December 2009 were also eligible for the extended program for their new hires in 2010. Hence it is more challenging to study the effects of the policy in 2010, as some firms treated in 2009 may not have been able to apply in 2010, because eligibility for the extended period was then based on the average size over 2009. As a consequence, we focus on outcomes in 2009.

present in the firm over the month. This means that fixed-term workers hired on the 15th of the month working full-time represent 0.5 employees. However, workers hired on permanent contracts are counted as 1 employee during the month no matter what day of the month they were hired on. All wage-earners working part-time, either on fixed-term or permanent contracts, are accounted *pro rata temporis* their regular number of hours during the month, excluding overtime hours. For instance, wage-earners working mornings only are counted as 0.5 employee.

13. The first announcement about a possible extension was made in the end of September 2009. This fact and the corresponding google search data shown on Figure 2, suggest that firms did not expect that the credit would be extended before the last quarter of 2009.

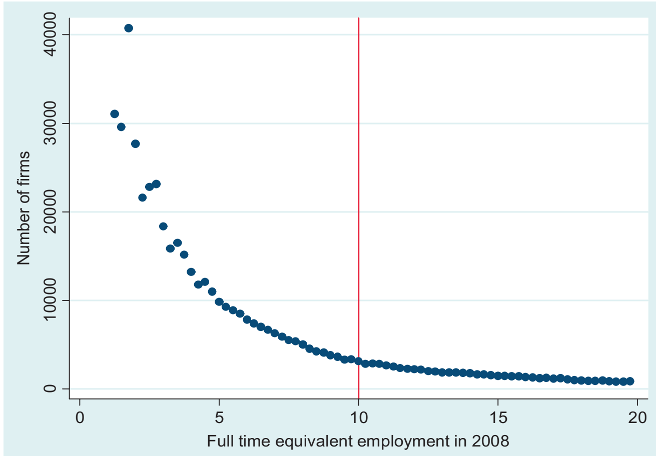

FIGURE 3

Firm size density.

The hiring credit was initially part of a wider array of policies—the stimulus package—designed to cope with the 2008–9 crisis. Within that array, this is the only item specifically targeted at small firms, and the only item directly altering the labour cost. The exact size threshold of 10 employees was mostly determined by the government budget constraints. Broadly speaking, there were no other explicit legal changes in this period that exerted a varying impact on firms with less or more than 10 employees.

While there are some minor discontinuities at the 10-employee threshold in the French legislation,[14] we do not see any accumulation of firms just below the threshold (Figure 3). This suggests that the changes in the labour cost or in the labour regulations at the threshold are not significant, nor salient enough to lead firms to sort. Such a sorting might have meant that firms below and above the threshold were reacting differently to the business cycles. This is in line with Ceci-Renaud and Chevalier (2010) who do not find any bunching at the 10-employee threshold. This contrasts with the accumulation of firms just below the 50-employee threshold, as reported by Gourio and Roys (2014) and Garicano *et al.* (2013). The difference in patterns around the 10 and 50-employee threshold is probably due to the greater change in costs at the 50-employee threshold and its greater saliency.[15] As Garicano *et al.* (2013, p. 14) put it, "Although there are some regulations that bind when a firm (or less often, a plant) reaches a lower threshold such as 10 or 20 employees, 50 is generally agreed by labour lawyers and business people to be the critical threshold when costs rise significantly".

14. An increase in the contribution rate for continuing vocational training of 0.55% to 1.05%, an obligation of monthly payments of social security contributions (instead of quarterly payments), an obligation for payment of transport subsidies and the loss of the possibility of a simplified balance sheet.

15. There are more substantial discontinuities at the 50-employee threshold in the French legislation, notably the possibility for employee unions of designating delegates, various obligations in the field of vocational training, the obligation to set up a works council, the obligation to set up a separate committee on health, safety and working conditions, the obligation to set up a "profits participation fund" in favor of employees, the obligation to set up "social plans", *i.e.* time-consuming and costly procedure in the event of redundancy involving nine or more employees, and the loss of the possibility of a simplified presentation of the financial statements.

3. DATA AND EMPIRICAL STRATEGY

3.1. *Data*

We use administrative data from two main distinct sources:

- the *Déclarations Administratives de Données Sociales* (DADS) built by the French Statistical Institute (INSEE) from the social contributions declarations of firms. Each year firms declare the employment spells, the number of hours worked, and the associated wages for each worker.
- the administrative file produced by the French Public Employment Service (*Pôle emploi*) which administered the payment of the subsidy, designated as the "hiring credit file". It contains information on the firms which enrolled in the *zéro charges* program, the level of the hiring wage, and the exact amount and duration of the subsidy received.

The DADS cover about 85% of French wage earners. Civil servants from the French central, regional and local administrations (general government) and workers from the public health care sector or employed by householders (*e.g.* for house-keeping or child care) do not appear in this employment register (until 2009). We append the employment registers from 2005 to 2009,[16] creating a panel of firms.[17] We restrict the sample to firms in the for-profit private sector and we drop the agricultural sector as well as associations. We also drop workers in temporary help agencies, as we do not know in which firms they actually work, as well as the 1% of firms with the highest employment growth rates in the sample. All relevant information pertaining to firm size, the number of hires, separations, the wage levels and the duration of contracts is taken from the DADS data set which describes the universe of firms relevant to our evaluation. We also compute the eligibility condition based on the size threshold (full-time equivalent) from the employment register, as the Public Employment Service computed it for the subsidized firms only.

Our two data sets can be matched using the firm identifier. This enables us to compute the take-up rate, which corresponds to the fraction of small firms actually benefiting from the hiring credit in 2009. The take-up rate amounts to 24%. This figure depends on the hiring rates of low-wage workers and on the take-up behaviour conditional on hiring low-wage workers, which we define as the attention rate. The attention rate (the share of subsidized hires among eligible hires with wages below 1.6 times the minimum wage and contract duration above one month) amounts to 47%. Figure 4 displays the take-up rate and the attention rate by firm size in 2008 (*i.e.* according to the eligibility criteria). The take-up rate sharply decreases for firms with eight employees or more and goes to zero for firms larger than 12 employees. Similarly the attention rate drops before the threshold and it is positive, around 3%, for firms with a workforce of 10–12 employees.

To the extent that, as discussed above, firms were not able to manipulate their size to meet the eligibility criterion, the drop in the attention rate before the threshold of 10 employees and the positive fraction of firms from 10 to 12 employees benefiting from the hiring credit are likely the consequences of measurement error. The eligibility criterion is difficult to measure precisely in the employment register at our disposal. In particular, according to the legal rules, workers hired on permanent contracts are considered to be present in the firm from the beginning of the month, even if they have been hired during the month. Since we only observe the type

16. The specification concerning the type of labour contract, either fixed-term or open-ended, is not available before 2005. Since the type of contract is used to compute the number of full time equivalent workers, as explained in footnote 12, we cannot use the DADS before 2005.

17. There is no permanent identifier for individual workers. Our data are not a panel of individual workers.

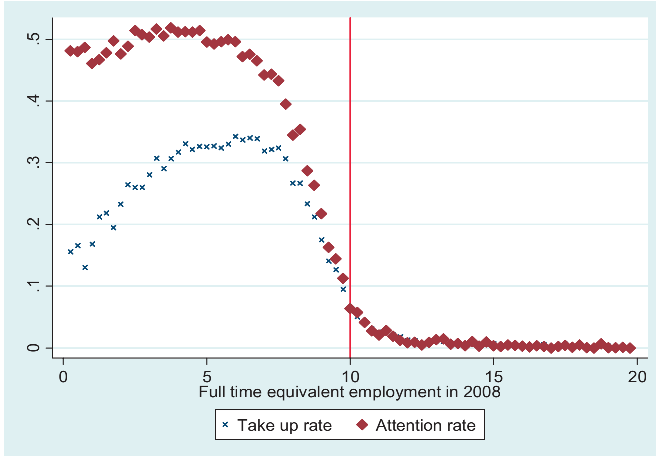

FIGURE 4

Fraction of firms and of hires that benefited from the hiring credit by firm size in 2009.

Notes: The take-up rate is the share of firms below 10 employees benefiting from the hiring credit in 2009; the attention rate is the share of hires with wages below 1.6 times the minimum wage lasting one month or more and which were subsidized in 2009. The number of full-time equivalent employees is measured over the first 11 months of 2008.

of contract at the end of the year for every worker, we are unable to know whether workers have been hired on permanent or temporary contracts because temporary contracts may have been converted into permanent contracts. Another reason could be that computing the eligibility criterion is a complex task, especially for small firms. Only ordinary employees are kept in the size computation, excluding apprentices and diverse categories of employees benefiting from other subsidies; employees contribute *pro rata temporis* but overtime hours are not taken into account. These features of the eligibility criteria may induce firms to overestimate their size and to refrain from claiming *zéro charges*. The resulting absence of discontinuity in the take-up rate prevents us from using a regression discontinuity design.

3.2. *Empirical strategy of the reduced-form approach*

The hiring credit may influence employment through its impact on hires and on separations. To see this, let us consider the law of motion of employment which determines the level of employment at the end of the current period

$$L = L_{-1} + H - S, \tag{1}$$

where L_{-1} stands for employment inherited from the previous period, H denotes the number of entries, and S is the number of separations.

Hiring credits aim at increasing employment through their effect on hires. However, it is possible that firms benefit from important amounts of hiring credits while the effects on net employment are negligible. Becker (2010) and Posner (2010), reacting to the Hiring Incentives to Restore Employment (HIRE) Act passed in the U.S. in 2010, argued that it will increase churning and wages with very little effect on employment. In our context, churning is potentially an important concern to the extent that worker flows in excess of those strictly necessary to achieve a given change in employment are large in France (Abowd *et al.*, 1999; Cahuc *et al.*, 2014).

If the hiring credit increases employment, it is nevertheless possible that its impact on hours worked is limited, because firms have incentives to substitute hours of subsidized employees for those of non-subsidized employees. Therefore, it is also important to analyse the response of hours of work.

In what follows, we estimate the impact of the hiring credit on employment, wages, hours of work, hires and separations, using two different identification strategies: a difference-in-differences strategy and an IV strategy. The difference-in-differences strategy contrasts the evolution of employment in firms with fewer than 10 employees and firms with more than 10 employees before and after the reform was implemented. The IV strategy contrasts the evolution of employment in employment pool $\times$ sector cells with high or low treatment intensity after the reform was implemented. We instrument the treatment intensity by the share of low-wage workers in the cell from 2006 to 2008 (before the reform).

4. DIFFERENCE-IN-DIFFERENCES

This section presents the difference-in-differences econometric model and our main results on the effects of the hiring credit on employment, hours worked, wages, hires, and separations.

4.1. *Econometric model*

We analyse yearly cohorts of firms. We select, for each cohort t, firms whose size criterion in year $t-1$ is around the cut-off (*i.e.* 10 full-time equivalent employees, calculated as the average of end-of-month *pro-rata temporis* headcounts between January and November of year $t-1$) and estimate the following difference-in-differences model:

$$Y_{it} = \alpha + \beta Z_{it} + \gamma D_t + \delta Z_{it} D_t + X_{it} b + u_{it}, \tag{2}$$

where Y_{it} is the outcome of firm i in period t, Z_{it} an eligibility dummy equal to 1 if the firm size in period $t-1$ is below 10, D_t a dummy for year 2009 when subsidies can be claimed, X_{it} a vector of covariates listed in the next section and in the note of Table 2. δ is our parameter of interest. It captures the differential evolution of the group targeted by the hiring credit. It can be interpreted as an Intention-To-Treat parameter. Accordingly, we refer, in this section, to firms with fewer than 10 employees in year $t-1$ as our "treatment" group, even if they do not claim the hiring credit. Note that because we define our eligibility dummy for every year, the treatment effect estimate is robust to potential mean-reversion bias that could occur if the definitions of the control and treatment groups had been based on the size of firms in 2008 only.[18]

In the benchmark estimations, the bandwidth goes from 6 (included) to 14 (excluded) full-time employees in the previous year.[19] In Table 1, we report characteristics of our 2009 cohort. These characteristics are measured in 2008. In the first three columns, we compare small and medium size firms. Small (*i.e.* eligible) firms operate less frequently in manufacturing industry and slightly more often in retail, transport, and merchant services than non-eligible medium size firms. They

18. Let us consider the simple difference-in-differences model, without controls, using the 2008 and 2009 cohorts of firms and with employment growth as an outcome. Then the difference between small and medium firms of the 2008 cohort writes: $\Delta = \mathbb{E}\left[\frac{L_{2008}-L_{2007}}{L_{2007}} | L_{2007} < 10\right] - \mathbb{E}\left[\frac{L_{2008}-L_{2007}}{L_{2007}} | L_{2007} > 10\right]$. If there is some mean-reversion bias around 10 employees, Δ would be strictly positive. Mean-reversion bias entails that the same difference using the 2009 cohort would be strictly positive even in the absence of treatment effect. However taking the difference-in-differences nets out the mean-reversion bias. Had we defined the treatment group according to the firm size in 2008, whatever the firm cohort, any mean-reversion bias would have contaminated our difference-in-differences estimate. Year placebo estimates in Supplementary Appendix C.1 confirm that our definition of the treatment group is robust to mean-reversion bias.

19. In Supplementary Appendix C.3, we present a robustness analysis where we vary the bandwidth around the 10-employee cutoff.

TABLE 1
The characteristics in 2008 of eligible/ineligible and subsidized/unsubsidized firms

No. of employees in 2008	Eligible 6–10	Ineligible 10–14	Diff test *p*-value	Subsidized 6–10	Not subsidized 6–10	Diff test *p*-value
Manufacturing	0.153	0.193	0.000	0.137	0.160	0.000
Construction	0.186	0.187	0.474	0.191	0.182	0.000
Retail and transport	0.307	0.288	0.000	0.319	0.302	0.000
Hotels and restaurants	0.100	0.091	0.000	0.143	0.083	0.000
Merchant services	0.254	0.244	0.000	0.208	0.272	0.000
Parisian area	0.241	0.229	0.000	0.154	0.275	0.000
North-West	0.245	0.254	0.000	0.265	0.237	0.000
North-East	0.120	0.124	0.032	0.128	0.116	0.000
South-East	0.268	0.264	0.185	0.307	0.253	0.000
South-West	0.127	0.129	0.382	0.146	0.119	0.000
Sales below 2 millions euros	0.449	0.204	0.000	0.519	0.422	0.000
Young firm (age below 5 years)	0.134	0.102	0.000	0.145	0.129	0.000
Mean share of ...						
... male managers	0.207	0.216	0.000	0.161	0.225	0.000
... female managers	0.121	0.115	0.000	0.105	0.127	0.000
... male white-collar	0.079	0.076	0.004	0.093	0.074	0.000
... female white-collar	0.210	0.187	0.000	0.253	0.194	0.000
... male blue-collar	0.347	0.364	0.000	0.352	0.344	0.006
... female blue-collar	0.036	0.041	0.000	0.035	0.036	0.655
Mean share of ...						
... low-wage workers	0.622	0.605	0.000	0.712	0.586	0.000
... part-time workers	0.256	0.208	0.000	0.258	0.255	0.039
No. of obs.	73,042	31,893	–	20,451	52,486	–

Notes: Low-wage workers earn between the minimum wage and 1.6 times this amount (on an hourly basis). Part-time workers work below 80% of normal working hours. The number of employees corresponds to the full-time equivalent in 2008 (average from 1 January to 30 November). The number of observations corresponds to the number of firms in the sample.
Source: DADS (Insee).

are slightly more frequently located in the Parisian area, and less frequently in the North West and the North East parts of France. Almost half of small firms have sales of less than 2 million euros, while four medium-size firms out of five exceed that mark. Small firms are also younger: 13% have existed for less than 5 years versus 10% for medium-size firms. The composition of the workforce (in 2008) differs between small and medium-sized firms. Small firms have more white-collar employees, while medium-sized firms have more blue-collar workers. Finally, the share of low-paid workers and that of part-time workers are both higher in small firms. These variables (lagged) are included in the regressions to control for these differences.

4.2. *Main results*

The validity of difference-in-differences estimations is heavily dependent on the common trend assumption. We describe the common trend for treated firms with previous size between 6 and 10 (excluded) and control firms with previous size from 10 to 14 in Figure 5. The outcome is average employment growth in each group.[20] Employment is computed at the firm level. Employment in year t is defined as employment on 30 November of year t. This ensures that employment in

20. We focus on the effect of the hiring credit on the growth rate of employment rather than on the employment level for the following reason. The common trend assumption on the employment level requires identical differences in employment levels between year t and year $t-1$ for the control and the treatment group before 2009, *i.e.* $\bar{L}_t^C - \bar{L}_{t-1}^C = \bar{L}_t^T - \bar{L}_{t-1}^T$ where $\bar{L}_t^j$ stands for average employment of group j ($j=C$ for the control group and $j=T$ for the treatment group) in year $t<2009$. We checked that this assumption is not fulfilled. This is not surprising inasmuch as the impact

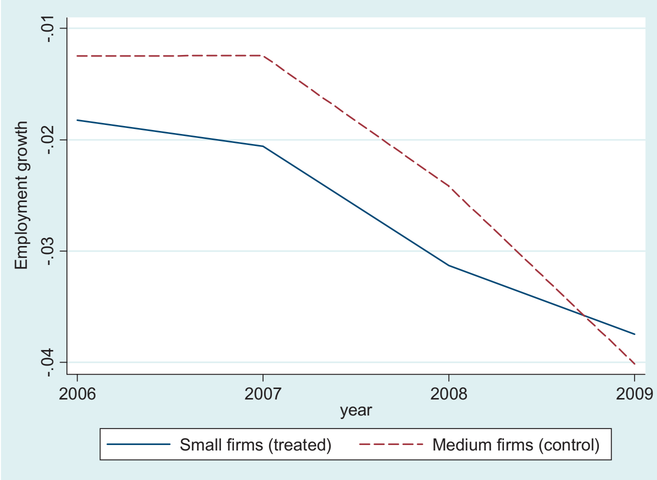

FIGURE 5

Average employment growth rate in firms in the treated and control groups.

Notes: Growth rate of employment between 30 November of year $t-1$ and year t. The treatment group comprises firms of size between 6 (included) and 10 (excluded) full-time equivalent employees in the previous year (average from 1 December to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full-time equivalent employees in the previous year (average from 1 December to 30 November).

2008 is not influenced by the hiring credit that was announced on 4 December 2008. Let $L_{i,t}$ denote employment in firm i on 30 November of year t; average employment growth for each group is $\frac{1}{N_t}\sum_i \frac{L_{i,t}-L_{i,t-1}}{L_{i,t-1}}$ where N_t is the number of firms in the group. Figure 5 shows that the difference in employment growth rates between the treatment group and the control group is negative and constant from 2006 to 2008. In 2009, this difference becomes positive: the growth rate of the treatment group drops by 0.9 percentage points while that of the control group drops by 1.6 percentage points.[21] Figure 6 shows that the same phenomenon arises for hours of work: the average growth rate of total hours of work per firm of the treatment group is below that of the control group from 2006 to 2008 and becomes larger than that of the control group in 2009. This points to positive treatment effects, that we estimate below.

In Table 2, we present our difference-in-differences estimates for different outcomes (in rows) and specifications (in columns). In column 1, our baseline sample comprises all cohorts from 2006 to 2009 without covariates. In column 2, we add covariates control which include year, sector

of productivity shocks or labour costs shocks on the employment level are expected to increase with the size of the firm. This is the case, for instance, when the wage elasticity of labour demand is constant. To see this, consider a simple static model, where the production function is $F(L)$ and the labour cost is equal to the net wage w times the labour wedge ϕ. The optimal level of employment satisfies $F'(L)=w\phi$. This equation implies that a one percent change in labour cost induces a change in employment level that is proportional to the initial employment level of the firm, *i.e.* $\mathrm{d}L=L\varepsilon\mathrm{d}\phi/\phi$, where $\varepsilon=F'(L)/LF''(L)$ denotes the elasticity of labour demand with respect to the labour cost $w\phi$. Note that taking log employment as outcome instead of the employment growth rate is valid only if the distribution of past employment is stable over cohorts of the control and the treatment groups. Running diff-in-diff on growth rates allows us to get estimates that are robust to potential changes in the distribution of past employment since the change in employment of each firm is proportional to its past size.

21. The average employment growth is negative for the treatment group and the control group all along the period. This is because new entrants, which typically account for a significant share of employment growth, are excluded from the sample. Bear in mind that, by construction, we cannot include new entrants since we study the behaviour of firms that had between 6 and 14 full time equivalent employees the previous year.

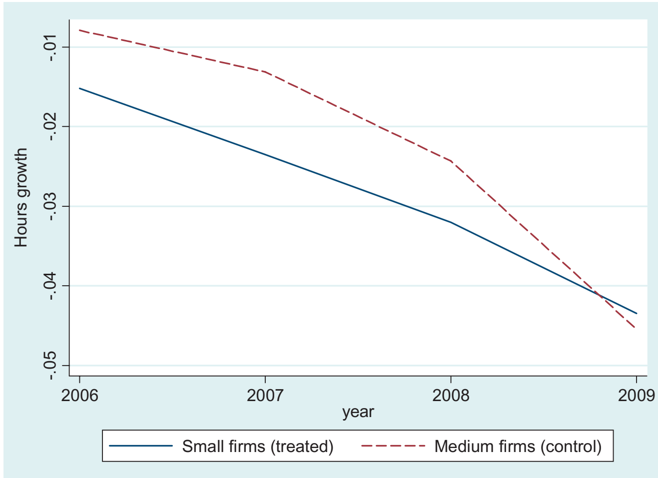

FIGURE 6

Average hours growth rate in firms in the treated and control groups.

Notes: Growth rate of the number of hours worked within each firm between November of year t and November of year $t-1$. The treatment group comprises firms of size between 6 (included) and 10 (excluded) full-time equivalent employees in the previous year (average from 1 December to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full-time equivalent employees in the previous year (average from 1 December to 30 November).

and regions dummies, as well as their interactions, dummies for firm age, for firms with sales below 2 million euros in the previous year, the share of low-wage and part-time workers in the previous year and the shares of female or male workers with different occupations (managers, white-collar or blue-collar workers). In column 3, we restrict the sample to cohorts 2008 and 2009 (to avoid potential specification errors related to underlying trends). To deal with serial correlation problems which could lead to misleadingly small standard errors (Bertrand *et al.*, 2004), we follow the approach suggested by Cameron and Miller (2015): in addition to the non-clustered robust standard errors, we compute the cluster-robust standard errors at progressively broader levels, starting at the firm level, then at the employment pool $\times$ sector unit level, and eventually at the employment pool level.

The results are very stable. They indicate that the hiring credit increased the employment growth rate of the treatment group by about 0.8 percentage point (column 2 of Table 2). Table 2 shows that the impact of the hiring credit on the growth of hours of work is similar to that on employment, indicating that firms did not reduce working hours on existing jobs to compensate for new hires. The last row of Table 2 shows that the hiring credit had no impact on the survival of firms, meaning that the hiring credit raised employment in surviving firms. Indeed, estimates on the subsample of surviving firms are identical to that of all firms, as shown in Table A11 in Supplementary Appendix.[22]

Table 3 displays separately the impact of the hiring credit on eligible jobs—jobs paying below 1.6 times the minimum wage that last at least one month—and on ineligible jobs.[23] The hiring

22. Our estimates are not weighted by firm size. This could bias our results if, for instance, the elasticity of labour demand depends on the size of firms. We checked that estimates provided in the course of the paper yield results similar to weighted estimates. This is illustrated by Table A12 in Supplementary Appendix which shows the weighted estimates corresponding to those displayed in Table 2.

23. The number of observations in Table 3 is smaller than in Table 2 because it excludes firms with eligible jobs only and firms with ineligible jobs only. The last column of Table 3 displays the difference-in-differences estimates for

TABLE 2

Difference-in-differences estimates of the impact of the hiring credit on various labour market outcomes in 2009

Cohorts	2006–9	2006–9	2008–9
Covariates	No	Yes	Yes
Emp. growth	0.009*** (0.002; 0.002; 0.002; 0.002)	0.008*** (0.002; 0.002; 0.002; 0.002)	0.009*** (0.002; 0.002; 0.002; 0.002)
Hours growth	0.010*** (0.002; 0.002; 0.002; 0.002)	0.009*** (0.002; 0.002; 0.002; 0.002)	0.008*** (0.002; 0.002; 0.002; 0.002)
Hiring rate	0.014*** (0.005; 0.004; 0.004; 0.004)	0.012*** (0.004; 0.004; 0.004; 0.004)	0.019*** (0.005; 0.004; 0.004; 0.004)
Sep. rate	0.005 (0.005; 0.004; 0.004; 0.004)	0.004 (0.004; 0.004; 0.004; 0.004)	0.010** (0.005; 0.004; 0.004; 0.004)
Survival rate	0.000 (0.001; 0.001; 0.001; 0.001)	0.000 (0.001; 0.001; 0.001; 0.001)	−0.001 (0.001; 0.001; 0.001; 0.001)
No. of Obs	406,468	406,468	207,379

Notes: This table presents our difference-in-differences estimates for different outcomes (rows) and different specifications (columns). The treatment group comprises firms of size between 6 (included) and 10 (excluded) full-time equivalent employees in the previous year (average from 1 January to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full-time equivalent employees in the previous year (average from 1 January to 30 November). We consider as outcomes the growth rate of employment between 30 November of year $t-1$ and year t; the growth rate of the number of hours worked between November of year $t-1$ and November of year t; the number of hires from 1 December of year $t-1$ to 30 November of year t divided by employment on 30 November of year $t-1$; the number of separations from 1 December of year $t-1$ to 30 November of year t divided by employment on 30 November of year $t-1$; the survival rate from 30 November year $t-1$ to 30 November year t. We define firms as survivors if they have at least one employee paid at the end of November in year t. As covariates, we include year, sector and regions dummies, as well as their interactions; we also include dummies for firm age, firms with sales below 2 million euros in the previous year, the share of low-wage and part-time workers in the previous year and the shares of female or male workers with different occupations (managers, white-collar or blue-collar workers). Standard errors in parentheses, respectively, not clustered, clustered at the firm level, at the employment pool X sector level, at the employment pool level. There are 348 employment pools ("zones d'emploi", Insee) and 5 sectors. *Significant at 10%, **Significant at 5%, ***Significant at 1%.
Source: DADS (Insee).

credit has a strong positive and significant impact on employment and hours for eligible jobs only. The impact for non eligible jobs is rather positive, but not significantly different from zero. This means that the hiring credit has had a positive impact on total employment and total hours mainly through its impact on eligible jobs, and very marginally on ineligible jobs.

4.3. *Robustness checks*

Our data set allows us to observe the evolution of employment month-by-month from 2006 to 2011. We use this information to exhibit the common trends of employment growth between small and medium-sized firms at the monthly level, before the introduction of the policy.

We compute the average employment growth from November of year $t-1$ to the end of month m in year t for both small and medium firms according to their size in year $t-1$. We denote the average employment growths for small and medium-sized firms by $g^1_{m,t}$ and $g^0_{m,t}$ respectively.

We then estimate, for every month from 2007 to 2009, the corresponding difference-in-differences coefficients: $g^1_{m,t}-g^0_{m,t}-(g^1_{m,t-1}-g^0_{m,t-1})$. Since there is no policy change from January 2007 to November 2008 (last month before the announcement of the policy), the difference-in-differences estimates provide placebo tests, as they should be 0 over this period.

all jobs with this smaller sample. Results are identical to those displayed in Table 2, corresponding to the full sample also comprising firms without jobs either below or above 1.6 times the minimum wage.

TABLE 3
Difference-in-differences estimates of the impact of the hiring credit for eligible and ineligible jobs on various labour market outcomes in 2009

	Eligible jobs	Ineligible jobs	All jobs
Employment growth	0.011***	0.002	0.008***
	(0.003)	(0.004)	(0.002)
Hours growth	0.012***	0.005	0.008***
	(0.003)	(0.004)	(0.002)
Hiring rate	0.011***	0.005	0.008**
	(0.004)	(0.008)	(0.004)
Separation rate	0.000	0.003	0.000
	(0.004)	(0.008)	(0.004)
No. of Observations	350,633	350,633	350,633

Notes: This table presents our difference-in-differences estimates for different outcomes (rows) and different types of jobs (columns): eligible jobs below 1.6 times the minimum wage that last at least one month; ineligible jobs above 1.6 times the minimum wage or that last less than one month; all jobs. The treatment group comprises firms of size between 6 (included) and 10 (excluded) full time equivalent employees in the previous year (average from 1 January to 30 November). The control group comprises firms of size between 10 (included) and 14 (excluded) full time equivalent employees in the previous year (average from 1 January to 30 November). We consider as outcomes the growth rate of employment between 30 November of year $t-1$ and year t; the growth rate of the number of hours worked between November of year $t-1$ and November of year t; the number of hires from 1 December of year $t-1$ to 30 November of year t divided by employment on 30 November of year $t-1$; the number of separations from 1 December of year $t-1$ to 30 November of year t divided by employment on 30 November of year $t-1$; as covariates, we include year, sector and regions dummies, as well as their interactions; we also include dummies for firm age, firms with sales below 2 million euros in the previous year, the share of low-wage and part-time workers in the previous year and the shares of female or male workers with different occupations (managers, white-collar, or blue-collar workers). Robust standard errors in parentheses. *Significant at 10%, **Significant at 5%, ***Significant at 1%.
Source: DADS (Insee).

Figure 7 shows that there is indeed no differential employment evolution between small and medium-sized firms. This figure is a further check of the common trend at annual frequency shown in Figure 5. Supplementary Appendix C provides year placebo tests on other outcomes, as well as other robustness checks (placebo size cutoffs, and changes in the selection of the estimation sample).

Figure 7 also sheds light on the adjustment of employment in 2009. The estimated impact of the hiring credit increases steadily over the year.[24] It is clear that *zéro charges* has had a significant and quick impact on employment, since the difference-in-differences estimates are positive, at 95% level of confidence, from the end of February 2009—three months after the start of the policy. This result is in line with the literature on dynamic labour demand, according to which employment reacts quickly to shocks on labour costs, with a delay that is clearly infra-annual (Hamermesh, 2013).

Figure 7 also reports difference-in-differences estimates for 2010 and 2011. To use only the pre-treatment period as reference to compute the differential evolution of the employment growth rate between small and medium sized firms over these two years, we then define the difference-in-differences estimate for month m of year t as $g^1_{m,t}-g^0_{m,t}-\left(g^1_{m,2008}-g^0_{m,2008}\right)$. It is important to remark that these difference-in-differences estimates do not allow us to evaluate the impact of zero charges properly. As explained above, the government announced on 16 November 2009 that the policy, which was initially designed to stop on 31 December 2009, was extended to hires occurring up to 30 June 2010. Firms below the average of 10 full-time equivalent employees from January 2009 to December 2009 were eligible for the extended program for their new hires in 2010. In this context, the difference-in-differences method is not appropriate to properly identify the effects of the policy in 2010, as some firms treated in 2009 may not have been able to apply in

24. It reaches 0.009 in November 2009, which is by construction consistent with Column 3 in Table 2.

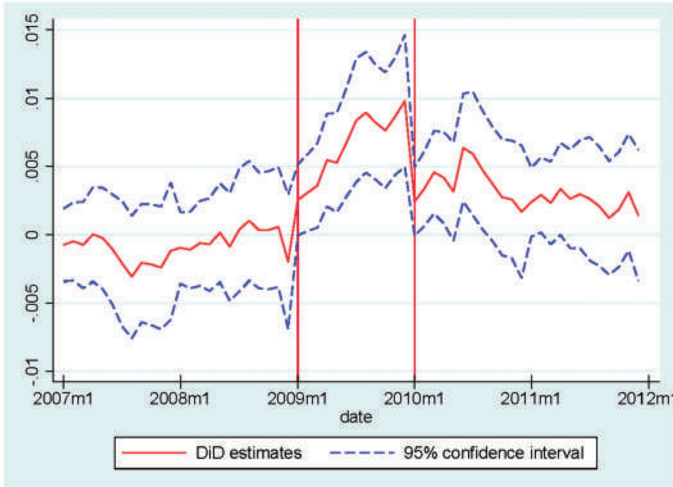

FIGURE 7

Difference-in-differences estimates on the employment growth rate at the monthly frequency.

Notes: Each point plots the monthly difference-in-differences estimate of employment growth rates between small and medium firms. The employment growth rate of month m of year t is the average employment growth from November of year $t-1$ to the end of month m in year t, for both small and medium-sized firms according to their size in year $t-1$. The DiD estimates of each year t is computed using relevant reference years. The reference year is 2006 for the 2007 DiD estimates, 2007 for the 2008 DiD estimates, and 2008 for DiD estimates of years 2009, 2010 and 2011. The estimates are obtained in a regression controlling for the same covariates as in column 3 of Table 2 and where standard errors are robust. The 2009 DiD estimates capture the impact of the 2009 hiring credits. The DiD estimates of years 2007 and 2008 are placebo tests of the common trend assumption. The DiD estimates of years 2010 and 2011 are potentially biased by endogenous selection into treatment groups for the 2010 extension of the hiring credit as discussed in the main text.

2010, because eligibility for the extended period was then based on the average size over 2009.[25] Moreover, firms could have manipulated the eligibility criteria for the 2010 extension in December 2009: firms above the 10 employees threshold in November 2009 had an incentive to downsize. For this reason, the paper is focused on the impact of *zéro charges* until November 2009.

That being said, Figure 7 shows that the difference-in-differences estimates are lower in 2010 than in 2009, and only marginally significant for the first semester in 2010. Small firms which already benefited from the hiring credit in 2009 did not experience any policy change in December 2009, and thus did not grow beyond their already high employment levels of 2009. This explains the sharp drop in the difference-in-differences estimates in January 2010, which capture the effect of the hiring credit on the employment growth from November 2009 to January 2010. Then, during the second semester of 2010, difference-in-differences estimates are not statistically significant. They are marginally lower than during the first semester, capturing the reaction of firms to the disappearance of the hiring credit. More interestingly, we do not find any significant intertemporal substitution effects, which would have implied significantly slower growth of small firms after the credit ends. However, insofar as the difference-in-differences estimates are probably biased after 2009, they are not relevant to properly identify intertemporal substitution effects either. Supplementary Appendix Table A13 comprises another test which points to the absence of substitution effects: it shows that there is no evidence of different employment effects of the hiring subsidy between service sectors and other sectors in which production can be more easily substituted from one period to another, thanks to storage.

25. Similarly, the policy extension announced in 2009 may have affected the firm size distribution in 2010 and consequently the split between medium and small size groups in the computation of the difference-in-difference estimates for 2011.

4.4. *Evaluation of the employment elasticity*

Since the hiring credit decreased the total labour cost of firms in the treatment group by 0.2%[26] and increased total employment by 0.8%, our estimates point to an employment elasticity with respect to the change in labour cost induced by the hiring credit of around −4, belonging to the 95% confidence interval [−6, −2].

The strong employment impact of zero charges relies on the absence of wage increases and on the absence of increased churning of workers, as shown in Supplementary Appendices A and B. Even if wages and labour turnover did not increase, this figure may at first sight seem incredibly high, compared to usual estimates of labour demand elasticities. For instance, Kramarz and Philippon (2001) and Abowd *et al.* (2006) found that the elasticity of employment with respect to the minimum wage is about −2 for men and −1.5 for women in France. Crépon and Desplatz (2001), using a different empirical strategy, found an elasticity equal to −0.8 for workers close to the minimum wage. The strong employment impact is not due to an intertemporal substitution effect, where firms would have frontloaded hires before the end of the first hiring credit period. We verify that the effect is still as strong in the service industries where production cannot be easily re-scheduled and buffered using inventories (see Supplementary Appendix Table A13).

The strong employment impact of *zéro charges* can be explained by the fact that a temporary decrease in average labour cost can have stronger employment effects when it is induced by a hiring credit than by wage changes that apply to all employees. To show this, let ε stand for the elasticity of contemporaneous employment, L, when the change in average labour cost per worker is due to a temporary change in the wage cost w of all incumbent and entrant workers. This is the standard definition of labour demand elasticity when the payroll equals wL. Let ε_σ stand for the elasticity of employment with respect to the average labour cost per worker when the change in average labour cost per worker is due to *zéro charges*. Bear in mind that the hiring credit alters the cost of entrants (*i.e.* new hires) only. The relation between employment and hires is given by the law of motion of employment (1). Let us assume that ε is identical in all firms and that the hiring credit does not increase churning of workers and wages, which is the case for *zéro charges* as shown in Supplementary Appendices A and B. We get (see Supplementary Appendix F.1.1 for computation details):

$$\varepsilon = \eta \varepsilon_\sigma, \tag{3}$$

where η is the share of employees that benefit from the hiring credit on 30 November 2009 in firms with positive take-up of *zéro charges*. As long as $\eta < 1$, the employment elasticity induced by the hiring credit is larger, in absolute value, than that induced by a proportional change in the wage cost of all workers. The reason is that subsidizing the jobs of incumbent workers yields no employment effects in firms that do not layoff workers absent the subsidy: all it does is to create windfalls for these firms. Using hiring credits is a means to target subsidies at marginal jobs, which yields positive employment effects, without providing subsidies to incumbent workers, which yields no employment effects in these firms.[27] In the limit case where $\eta = 1$ (which would happen if the take-up rate were 100% and the whole workforce were renewed over the course of

26. In November 2009, firms in the treatment group got 3.6 million euros from *zéro charges* while their labour cost during that month was 1.75 billion euros, which corresponds to a decrease of 0.21% in labour cost. Over the course of year 2009, *zéro charges* decreased the labour cost of firms in the treatment group by 0.14%. The amount of subsidies paid by *zéro charges* increased progressively during 2009.

27. It should be noticed that wage subsidies can lower job destruction (Schoefer, 2015). Since most job separations affect workers of low tenure, who potentially benefit from the hiring credit, the differential impact of hiring credits and wage subsidies on job separations is an open question left for future research.

a year), the two elasticities are identical because the entire workforce of firms that benefit from the hiring credit is subsidized.

All in all, we find that ε, the elasticity of employment with respect to labour cost induced by a change in wage, is smaller, in absolute value, than when the labour cost is modified by the hiring credit. The 95% interval confidence of the elasticity ε is $[-1.5,-0.5]$,[28] which is in line with previous estimates obtained for France. We show in Supplementary Appendix C.3 that this estimate is likely a lower bound for the absolute value of the elasticity of employment, which becomes larger when the difference-in-differences model is estimated with bandwidths that exclude firms with positive take-up rates from the control group.

5. IV STRATEGY: VARIATIONS IN TREATMENT INTENSITY ACROSS LABOUR MARKETS

The validity of the difference-in-differences identification strategy relies on the assumption that medium-sized firms—our control group—are not affected by the policy. This assumption is questionable. First, the supplementary hires induced by the hiring credit might increase labour market tightness (vacancies/unemployment ratio) and thus the recruiting costs for all firms. Second, the hiring credit may also increase wages in all firms. In our context, it is unlikely that the control group has been affected by the hiring credit to the extent that the hiring credit subsidized about 1% of all jobs only in the economy.[29] Nevertheless, to deal with this issue, we implement an alternative identification strategy which does not rely on the assumption that medium-sized firms have not been impacted by the policy. We exploit variations in the treatment intensity across local sector-specific labour markets.

5.1. *Econometric model*

We group firms into cells wherein the intensity of the treatment differed. Cells are defined as sectors in employment pools (or commuting zones). We distinguish 348 employment pools,[30] and 5 sectors (manufacturing, construction, retail and transport, hotels and restaurants, and other merchant services). We propose to use variation in treatment intensity across cells to estimate the impact of the hiring credit on employment among eligible firms. Treatment intensity can be measured by the share of subsidized hires among small firms in each cell. In cells where there is a larger share of subsidized hires among small firms, *zéro charges* should entail a larger effect on the growth rate of employment among these firms.[31] Formally, our equation of interest is:

$$Y_{jk}^{2009}=\alpha+\delta I_{jk}+\gamma Y_{jk}^{2008}+\beta X_{jk}+v_{jk}, \tag{4}$$

28. The share of employees that benefit from the hiring credit on 30 November 2009 in firms with positive take-up of *zéro charges* amounts to 0.26 which implies that $\varepsilon=-4\times 0.26\simeq -1$ with a 95% confidence interval equal to $[-1.5,-0.5]$.

29. About 20% of workers are employed in firms with fewer than 10 employees. The take-up rate of *zéro charges* amounts to 24%, and 26% of workers employed on 30 November 2009 in firms with positive take-up benefit from the hiring credit. Thus, the share of jobs that benefit from *zéro charges* in November 2009 was equal to $0.20\times 0.24\times 0.26=1.2\%$.

30. We use the 348 *zones d'emploi* provided by INSEE, the French national statistical office. A *zone d'emploi* is a geographic area wherein most workers reside and work, and in which companies can find most of the labour needed for the jobs offered. The definition of *zone d'emploi* is based on the flow of commuting workers observed in the 2006 Census.

31. This design was used to evaluate the effect of the federal minimum wage in the U.S. using cross-state variation in the fraction of low wage workers (Card, 1992).

where Y_{jk}^{2009} is the employment growth rate[32] between November 2008 and November 2009 averaged across eligible firms (with 6 to 10 employees in 2008) in commuting zone j and producing in sector k, Y_{jk}^{2008} is the lagged outcome, I_{jk} is the share of subsidized hires among all hires in 2009 in small firms and captures the treatment intensity, X_{jk} is a set of controls at the cell level.

The share of subsidized hires may be correlated with the error term v_{jk} to the extent that the take-up rate is endogenous. As an instrument for the treatment intensity, we use the share of eligible hires (below 1.6 the minimum wage) in small firms averaged from 2006 to 2008. Note that this share is measured before the introduction of the program. The share of eligible hires in 2009 would probably be more correlated with treatment intensity but it is also endogenous, since firms can always shift their wage offers to be eligible for the hiring credit. Therefore it cannot be used as an instrument.

One concern with our instrument could be that it also affects employment growth directly. For example, this would be the case if the business cycle has differential effects for low-wage or high-wage workers. To limit this concern, we include, as an independent variable, a shift-share prediction of employment growth from 2008 to 2009 in the same cell. In every cell jk, we measure the 2008 share of employment in the sub-industries l of sector k, which we denote S_{jk}^{l}. Then, for every two-digit industry l, we compute the national employment growth from 2008 to 2009 in firms below 50 employees, excluding the commuting zone j: g_{-j}^{l}. The shift-share prediction writes: $\Sigma_{l\in k}\, S_{jk}^{l} g_{-j}^{l}$. To further control for local shocks, we introduce as independent variables the lagged employment growth from 2007 to 2008 of small firms in the commuting zones around the cell of interest, both producing in the same sector and in the other sectors. The set of controls X_{jk} also includes sector dummies and some key cell characteristics measured in the previous year as in the difference-in-differences strategy: the distribution of firms' age, the (log) cell size, the share of firms with sales below 2 million euros, the share of part-time workers and the share of female/male executive/white-collar/blue-collar workers. It also includes cell-level measures of sales/value-added/dividends per employee and of financial leverage (*i.e.* debt over equity), averaged over the pre-policy period from 2005 to 2008.[33] These balance sheet data come from the French tax returns FICUS/FARE.

The IV strategy can only succeed if there is variation in the share of low-pay hires across employment pool $\times$ sector cells. This is the case as shown in Figure 8, which displays the distribution of this share among the 348 employment pool $\times$ 5 sector cells in our sample (*i.e.* around 1720 observations, in total, given that some pools do not feature all sectors). The 2006–8 average share of eligible hires among small firms (with 6 to 10 employees to be consistent with our baseline estimates) averages 0.575 with a standard deviation of 0.093.

5.2. *Results*

We first estimate equation (4) using weighted OLS, where weights are the employment size of each cell as measured among 6–10 employees firms in 2008. Columns 1 and 2 of Table 4 show that a higher share of subsidized hires is associated with a higher growth rate of employment. However, the presence of confounding variables, influencing the employment growth rate and the share of subsidized hires, can imply that OLS estimates are biased. For instance, commuting zones where

32. Henceforth, we focus on employment for the sake of clarity. Results on hours of work, which are not statistically different from those obtained on employment, are consistent with those obtained with the difference-in-differences strategy.

33. The debt-over-equity ratio is a commonly-used measure of financial leverage in the literature (see the recent example in Giroud and Mueller, 2017).

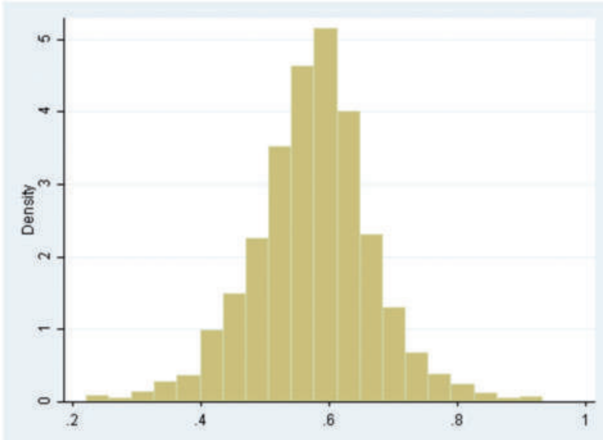

FIGURE 8
Density of the share of eligible hires from 2006 to 2008 among small firms.

TABLE 4
Cross-unit estimates of the impact of the hiring credit on employment growth rates in 2009, based on OLS and IV

	OLS		IV-2SLS	
Covariates	No	Yes	No	Yes
			First stage	
			Share of subsidized hires (I_{jk})	
Share of eligible hires in 2006-2008			0.519***	0.267***
			(0.025)	(0.036)
R^2			0.294	0.392
			Second stage	
		Employment growth		
Share of subsidized hires (I_{jk})	0.029**	0.028**	0.067***	0.138**
	(0.012)	(0.012)	(0.020)	(0.069)
R^2	0.006	0.057		
No. of Observations	1,721	1,721	1,721	1,721

Notes: This table presents estimates based on cross-cell differences in the intensity of treatment in 2009. Each cell corresponds to one employment pool and one sector. Within each cell the treatment group comprises firms of size between 6 (included) and 10 (excluded) full-time equivalent employees in the previous year (average from 1 January to 30 November). Each cell is weighted by its relative employment size as measured among 6–10 employees firms. We consider as outcome the growth rate of employment between 30 November of 2008 and 2009. We control for the lagged employment growth rate from 2007 to 2008. The share of subsidized hires in 2009 is instrumented by the average share of eligible hires (*i.e.* hires in small firms with wages between the minimum wage and 1.6 times the minimum wage) in the corresponding cell from 2006 to 2008. Covariates include a shift-share prediction of employment growth from 2008 to 2009 in the same cell, and the lagged employment growth from 2007 to 2008 of small firms in the commuting zones around the cell of interest. The set of controls also includes sector dummies and some key cell characteristics measured in the previous year: the distribution of firms' age, the (log) cell size, the share of firms with sales below 2 million euros, the share of part-time workers and the share of female/male executive/white-collar/blue-collar workers. It also includes cell-level measures of sales/value-added/dividends per employee and of financial leverage (debt over equity), averaged over the pre-policy period from 2005 to 2008. Robust standard errors in parentheses. *Significant at 10%, **Significant at 5%, ***Significant at 1%.
Source: DADS and FICUS/FARE (Insee), Pole emploi.

entrepreneurs are more dynamic may create more jobs and may have lower take-up rates of the hiring credit because their opportunity cost of applying for the hiring credit is higher. Columns 3 and 4 of Table 4 show the results of the IV estimation. The upper panel reports the estimates of the first stage. It shows that the share of eligible hires from 2006 to 2008 is a strong predictor of the treatment intensity in 2009. The coefficients of the second-stage estimation, reported in the bottom panel, are larger than those obtained with OLS, and they are consistent with the estimates obtained with the difference-in-differences strategy.[34] We checked that restricting the sample to cells with at least 10, or 30 firms between 6 and 10 employees, does not alter these results. Supplementary Appendix C provides year placebo tests.

To check further that our two identification strategies yield consistent results, we regress the difference-in-differences equation (2) on the sample made of the employment pool $\times$ sector units. To do so, for each unit and each year we compute the average growth rates of employment and hours worked separately for the two groups of firms (treatment or control). We weight each employment pool $\times$ sector unit by its employment size among firms from 6 to 14 full-time equivalent employees in the previous year. The results, in Supplementary Appendix Table A14, are similar to those of Table 2. If we restrict our sample to employment pool $\times$ sector units with at least 10, or 20 firms with 6 to 10 employees present in a given year, results remain unchanged.

6. EQUILIBRIUM EFFECTS

Our previous results suggest that medium-sized firms have not been affected by the hiring credit targeted at small-sized firms to the extent that we find consistent results with the difference-in-differences and the IV identification strategies. Nevertheless, to shed more light on this issue, we check whether employment and wages of firms above 10 employees, as well as the duration of vacancies have been impacted by the share of subsidized hires in their employment pool $\times$ sector cell. If there are equilibrium effects that reduce the net impact of the hiring credit, we should observe lower growth rates of employment among non-eligible firms in cells with a higher share of subsidized hires. Similarly, we should observe either higher wage growth or longer vacancy duration in cells with a higher share of subsidized hires. We adopt the same definition of cells as in the previous section (348 commuting zones $\times$ 5 sectors).

Within each of the $1,720$ employment pool $\times$ sector units for which we have observations in both the treatment and the control groups for 2008 and 2009 (*i.e.* 1675 observations, in total), we compute the ratio of subsidized hires in 2009 to all hires observed in the same year among firms with 0 to 20 full-time equivalent employees in 2008, denoted S_{jk}, where j stands for the employment pool and k for the sector. The average value of S_{jk}, is 0.182, and its standard deviation is 0.085. We also compute for each unit the average growth rate of employment and of wages from November 2008 to November 2009 among firms having from 10 to 20 full-time equivalent employees in the previous year.[35] Using data on vacancies posted at the French Public Employment Service, we compute the average time needed to fill vacancies posted in 2009 for each unit. We then compare the labour market outcomes across units with different shares of subsidized hires. To achieve this, we estimate the following model:

$$Y_{jk}^{2009} = \alpha + \lambda S_{jk} + \gamma Y_{jk}^{2008} + \beta X_{jk} + u_{jk}, \tag{5}$$

where Y_{jk}^{2009} stands for our outcome of interest in 2009 in employment pool j and sector k, Y_{jk}^{2008} its value in the previous year, S_{jk} is the share of hires in the cell subsidized by the 2009 hiring

34. To compare these estimates with the benchmark intention to treat estimates obtained in Table 2, one needs to account for the share of subsidized hires among small firms. In our sample, the weighted average value of this share is 0.17. Using the value of the coefficient of the fourth column of Table 4 this leads to effects at the mean value of $0.17 \times 0.138 = 0.023$ on the employment growth rate with 95% interval confidence $[0.001, 0.046]$.

35. To control for differential selection into employment across unit, we restrict the wage computation to workers who were both employed in 2008 and in 2009 (see Supplementary Appendix A for more details on wages data).

TABLE 5

Cross-unit estimates of equilibrium effects on the employment and wage growth rates of firms with 10–20 employees and on the vacancy duration, based on OLS and IV

	OLS		IV 2SLS - Second stage	
	(1)	(2)	(3)	(4)
Employment growth	0.073***	0.027	0.118***	0.008
	(0.017)	(0.022)	(0.023)	(0.051)
Wage growth	0.110***	0.023*	0.185***	0.055
	(0.021)	(0.012)	(0.026)	(0.035)
Vacancy duration (log)	−0.184	0.033	−0.529***	−0.037
	(0.145)	(0.054)	(0.157)	(0.142)
Covariates	No	Yes	No	Yes
No. of Observations	1,675	1,675	1,675	1,675

Notes: This table presents estimates based on cross-cell difference in treatment intensity in 2009. Each cell is one sector in one commuting zone. The treatment intensity is measured as the ratio of subsidized hires in 2009 over total hires in the same year among firms smaller than 20 employees. The dependent variable is either the employment growth of firms sized 10–20 in the previous year (line 1), or the wage growth of their employees (line 2), or the average vacancy duration in 2009 in the cell (line 3). In the IV regressions, the treatment intensity is instrumented by the corresponding shares of eligible hires, *i.e.* the ratios of the eligible hires in 2008 to total hires in the same year among firms with 0 to 20 full-time employees in the previous year, averaged from 2006 to 2008. Covariates include a shift-share prediction of employment growth from 2008 to 2009 in the same cell, and the lagged outcome. The set of controls also includes sector dummies and some key cell characteristics measured in 2008: the distribution of firms' age, the (log) cell size, the share of firms with sales below 2 million euros, the share of part-time workers and the share of female/male executive/white-collar/blue-collar workers. It also includes cell-level measures of sales/value-added/dividends per employee and of financial leverage (debt over equity), averaged over the pre-policy period from 2005 to 2008. Weights are used: for each employment pool x sector unit the weight equals total employment among firms with less than 14 full-time equivalent employees in the previous year. Robust standard errors in parentheses. *Significant at 10%, **Significant at 5%, ***Significant at 1%. *Source*: DADS and FICUS/FARE (Insee), Pole emploi.

credit. The term u_{jk} is a residual. We include the same set of control variables X_{jk} as in the IV strategy of the previous section.

As in the previous section, the number of subsidized hires in 2009 might be affected by unobserved shocks that also affect employment of medium-sized firms, meaning that the ratio S_{jk} of subsidized hires in 2009 is potentially endogenous in equation (5). For this reason, the ratio S_{jk} of subsidized hires in 2009 is instrumented by the corresponding average share of eligible hires from 2006 to 2008 (when the subsidy was not yet implemented), denoted E_{jk}.

Columns 1 and 2 of Table 5 present the results of the OLS estimation of equation (5), one row for each outcome. Columns 3 and 4 present the results of the IV estimation. Focusing on the results for employment growth and wage growth (first two rows), there is a statistically significant positive correlation between the share of subsidized hires in 2009 and the growth rates of employment and wages in 2009, when we do not add any controls (column 1). Without any controls, the IV estimates are also statistically significant (column 3). However, as we add controls, the OLS and IV estimates are smaller and no longer statistically significant—except at the 10% level for the OLS estimate on wages. Concerning the vacancy duration, we do not find any statistically significant effect of the local share of subsidized hires, once we add controls and instrument the share of subsidized hires. Table 6, which presents the first stage of the IV estimation shows that the shares of subsidized hires in 2009 are strongly correlated with the instruments. These results remain unchanged if we restrict our sample to all employment pool × sector cells with at least 10 or 30 firms. In Supplementary Appendix C.4, we show that we do not find any equilibrium effects when we allow the spillovers to go beyond the boundaries of the commuting zone × sector cell. All in all, these results suggest that the hiring credit had no impact on the medium-sized firms.

TABLE 6
First stage of the instrumental variable estimates of equilibrium effects

IV 2SLS - First stage		
	Share of subsidized hires (S_{jk})	
Share of eligible hires in 2006–8 (E_{jk})	0.597*** (0.028)	0.439*** (0.042)
R^2	0.478	0.593
Covariates	No	Yes
No. of Observations	1,675	1,675

Notes: This table presents the first stage of the IV estimation of equation (5). We regress, at the cell level, the share of subsidized hires in 2009 on the share of eligible hires in the same year—among firms smaller than 20 employees. We use the same covariates and weights, as in Table 5. Robust standard errors in parentheses. *Significant at 10%, **Significant at 5%, ***Significant at 1%.
Source: DADS and FICUS/FARE (Insee), Pole emploi.

7. COSTS PER JOB CREATED

In this section, we first compute the cost per job created by *zéro charges* under the assumption that there are no equilibrium effects. This assumption is consistent with the results of Section 6. Only a small share of firms were eligible for this temporary hiring credit, in a context where the minimum wage was binding and unemployment was high, so that we do not observe any equilibrium effects. In the second part of this section, we estimate the parameters of a structural search and matching model to simulate how equilibrium effects could affect the cost per job created, if the hiring credit were implemented in different economic environments, at different scales and on different time spans. A potential concern with this approach is that the estimation of the structural parameters of the search and matching model relies on the behaviour of small-sized firms following a shock on low wages. In this context, empirical evidence suggests that our estimates would rather yield a lower bound of cost per job created.[36]

7.1. *Cost per job created by zéro charges*

Based on our estimates, it is possible to compute the gross cost per job created by the hiring credit in the treatment group. The cost per job created by the hiring credit, c_σ, is a simple function of the contemporaneous wage w and of the employment elasticity with respect to the change in average labour cost induced by the hiring credit (see Supplementary Appendix F.1.1):

$$c_\sigma = -\frac{w}{\varepsilon_\sigma}. \tag{6}$$

Since we estimated that $\varepsilon_\sigma = -4$, this formula also indicates that the cost per job created is equal to 25% of the cost of a job. Accordingly, at the end of 2009, the monthly cost of creating one job amounts to around 700 euros.[37]

36. Empirical evidence shows that the labour demand for low-skilled workers is more elastic (Hamermesh, 2013; Lichter *et al.* 2015). Therefore, if the credit had concerned all types of workers, we expect the cost per job created to be higher. Second, there is far less empirical evidence on the heterogeneity of labour demand elasticity by firm size. That being said, we can expect large firms to be more capital intensive. The standard labour demand theory of Cobb–Douglas firms predicts that the short-run labour demand elasticity decreases with the capital intensity of the firm. Then the cost per job created estimated on small firms is probably a lower bound of the cost of a similar hiring credits, independent of firm size.

37. Alternatively, we can directly compute the cost per job created from the treatment effect estimate. There are 646,717 jobs in the treatment group at the end of 2008 (cf. Supplementary Appendix). According to Table 2, our estimate

This is a gross cost, because it ignores the savings generated by job creation in terms of unemployment and other social benefits that would have been paid in the absence of the measure. It also ignores the remaining social contributions paid by employees on these additional jobs. We exploit a survey, presented in Supplementary Appendix D, which allows us to precisely evaluate the savings permitted by *zéro charges* on social benefits. To this end we rely on two key assumptions. First, consistent with our estimation of the impact of *zéro charges* on net job creation, we assume that the number of jobs created by *zéro charges* reduced non-employment by the same amount. Second, we assume that social benefits would have been paid to individuals identical to the beneficiaries of *zéro charges* if they had remained on the dole. We find that the savings amount to about 700 euros per month. This makes the net cost of the hiring credit per created job equal to zero.[38]

7.2. *The search and matching model*

The previous estimated costs hold for a small and temporary hiring credit program which subsidized about 1% of all jobs in the economy and had no equilibrium effects. Moreover, this hiring credit was implemented during a recession, in a context where the minimum wage was binding and unemployment was high. A hiring credit covering more hires or occurring in different environments might entail equilibrium effects which could change significantly the impact of the policy, as suggested by Beaudry *et al.* (2012, 2014), Crépon *et al.* (2013), Gautier *et al.* (2012), and Lalive *et al.* (2013). To shed light on this issue, we estimate a search and matching model which enables us to account for equilibrium effects.

7.2.1. Model description. We consider a discrete time economy with a large number of labour pools or "islands" in which a representative firm produces a differentiated product with labour only as in the Dixit and Stiglitz (1977) model. The revenue function of the firm in period t is equal to $A_tR(L_t)$, where R is an increasing and concave function homogeneous of degree $(1+\alpha)$, such that $R(L_t)=(L_t)^{1+\alpha}/(1+\alpha)$, and $A_t>0$ is a productivity parameter. In this set-up, parameter α is a composite of the labour elasticity of the production function and of the price elasticity of the demand function for the product of the firm, as shown in Supplementary Appendix F.1.1.

The firm needs to post vacant jobs to hire workers. A vacant job costs c_V units of output per period. In each period, the sequence of decisions is as follows: (1) an exogenous proportion q_{t-1} of workers quit the firm; (2) vacant jobs are posted; (3) workers are hired; (4) production takes place and wages are paid. The assumption of exogenous job separation, which allows us to simplify the analysis, is consistent with the finding that the hiring credit did not induce firms to increase labour turnover to benefit from the subsidy (Supplementary Appendix B).

A vacant job posted in period t is matched with a worker with probability $m_t\in[0,1]$ in the period and remains vacant with probability $1-m_t$. The probability to fill a vacant job is determined by a matching function: $m(\theta_t)=\bar{m}\theta_t^{-\mu}$, where $\theta_t=V_t/U_{t-1}$, equal to the ratio of the number of job vacancies V_t over the number of unemployed workers U_{t-1}, denotes the labour

of coefficient δ when the dependent variable is $\Delta L/L_{-1}$ in equation (2), equals 0.008. Thus, the number of jobs created in the treatment group is $0.008\times 646,717=5,173$. The *zéro charges* hiring credit provided 3.6 million euros to the firms of the treatment group in our sample at the end of the period, *i.e.* in November 2009. Accordingly, at the end of 2009, the monthly cost of creating one job amounts to 700 euros.

38. These costs do not account for the potential costs associated with distortive taxes used to finance the hiring credit.

market tightness; $\bar{m}$ is a positive parameter, and μ, the elasticity of matching with respect to unemployment, belongs to the interval $(0,1)$.

Exogenous wages

Let us denote by w_t the wage in period t and by $\Pi(Z_t, L_{t-1})$ the value function of the firm, where $Z_t = (A_t, w_t, m_t, q_{t-1})$. Let β denote the discount factor and $\mathbb{E}_t$ the expectation operator. When wages are exogenous, the value function of the firm satisfies

$$\Pi(Z_t, L_{t-1}) = \max_{V_t} A_t R(L_t) - w_t L_t - c_V V_t + \beta \mathbb{E}_t \Pi(Z_{t+1}, L_t)$$

subject to the law of motion of employment:

$$L_t = (1-q_{t-1})L_{t-1} + m_t V_t.$$

The solution of the firm's problem is detailed in Supplementary Appendix F. When the wage is exogenous (see Supplementary Appendix F.1), the equilibrium values of employment and of the labour market tightness are defined by the labour demand equation derived from the solution of the maximization problem of the firm, and by the law of motion of the unemployment rate $u_t = 1 - L_t$:

$$A_t R_L(L_t) = w_t + \frac{c_V}{m(\theta_t)} - \beta(1-q_t)\mathbb{E}_t \frac{c_V}{m(\theta_{t+1})} \tag{7}$$

$$u_t = u_{t-1} + q_{t-1}(1-u_{t-1}) - \theta_t m(\theta_t) u_{t-1}. \tag{8}$$

Endogenous wages

The case with endogenous wage is described by a directed search and matching model with wage posting in the spirit of Moen (1997) and Kaas and Kircher (2015). In each labour pool, employers post vacant jobs. Each job vacancy is posted with a non renegotiable wage which applies throughout the employer–employee relationship. Unemployed workers are assumed to have perfect information on the situation in each labour pool. Their search activity can be directed towards their preferred employment pool. The mobility of workers between labour pools is perfect. The solutions to the model and the computation of the cost per job created are presented in Supplementary Appendix F.2.

The assumption of wage posting also implies that the market equilibrium allocation is constrained efficient if the product market is perfectly competitive. To give room to welfare improving hiring subsidies, we assume that there is imperfect competition on the product market. This ensures that the market equilibrium unemployment rate is higher than the constrained efficient unemployment rate.[39]

7.2.2. Structural estimation. Our objective is to estimate the parameters μ, c_V and $\alpha = LR_{LL}(L)/R_L(L)$, *i.e.* the elasticity of the marginal revenue with respect to labour. It is shown, in

39. An alternative possibility would have been to assume perfect competition on the product market and wage bargaining. However, wage bargaining in large (*i.e.* with more than one employee) firms with decreasing marginal productivity of labour induces over-employment, as shown by Stole and Zwiebel (1996) and Cahuc *et al.* (2008). Accordingly, assuming wage bargaining in a context with large firms does not produce inefficiently high unemployment, leaving less scope for welfare improving hiring credits.

TABLE 7
Estimates of the parameters of the matching function

Dep. var.	Job finding rate (log)	
	(1) OLS	(2) IV
Labour market tightness (log)	0.654*** (0.018)	0.545*** (0.060)
Year FE	Yes	Yes
Employment pool FE	Yes	Yes
No. of Observations	1,392	1,044

Notes: Estimation of the parameter of the job matching function equation (A15) on 348 employment pools from 2006 to 2009. (1) Standard (within) OLS; (2) IV regression. As an instrument we use the lagged value of the labour market tightness. Robust standard errors in parentheses. *Significant at 10%, **Significant at 5%, ***Significant at 1%.
Source: Pôle emploi and Enquête emploi (Labour Force Survey, Insee).

Supplementary Appendix F, that these parameters, together with the discount factor β, the job separation rate q_t, the duration of job vacancies, $1/m(\theta_t)$, and the unemployment rate u_t, allow us to compute the elasticity of employment with respect to different types of hiring credit: temporary, permanent, targeted at a small subset of jobs, generalized to all jobs.

Following the search and matching literature (*e.g.* Kaas and Kircher, 2015; Hall, 2017), the annual discount factor β is set at 0.95. The other statistics—$q_t, 1/m(\theta_t), u_t$—are directly observable in our data. We conduct the structural estimation of parameters μ, c_V and α in two steps—see Supplementary Appendix E for more details on the data and on the estimation.

Matching technology

First, using data on job finding rates and tightness at the employment pool level from the Public Employment Service and from the Labour Force Survey, we estimate the parameters of the matching technology. The elasticity μ is identified using variations of job finding rates and labour market tightness over time within employment pools. In the spirit of Borowczyk-Martins *et al.* (2013), we address potential endogeneity issues using an IV strategy based on past values of the labour market tightness. The results of the estimation are presented in Table 7. We obtain an elasticity of the matching function with respect to the unemployment rate, μ, around 0.45.

Revenue function and vacancy posting cost

Second, we estimate the production parameters, namely the elasticity of the marginal revenue with respect to labour, α, and the cost of posting a vacancy c_V. These parameters are obtained by conducting a structural estimation of the labour demand equation (7). The identification strategy, which relies on the natural experiment triggered by *zéro charges*, is similar to the IV strategy developed in section 5. We focus on small firms, which are eligible for the program. The policy shock entails exogenous variations in labour cost that are used to identify the revenue parameter α. In employment pools where the shock on labour cost is larger because the share of hires eligible for the hiring credit is larger, average employment effects should also be larger. These differences in employment effects yield the elasticity of the marginal revenue α. The estimation strategy is complemented to as well recover the vacancy posting cost c_V. The effects of the hiring credit on employment are larger where baseline hiring costs are lower. Hiring costs depend on the time needed to fill a vacancy. In a tight labour market, where vacancy duration is large, the hiring credit is less effective. Using heterogeneity in the effects of the hiring credits across local labour markets with low or large vacancy duration, we identify the vacancy posting cost c_V. Both parameters—the elasticity and the vacancy cost—are identified using the average values over 2006–8 in firms with 6 to 10 full-time employees of the share of eligible hires, of the duration

TABLE 8
Estimates of the parameters of the revenue function and of the vacancy posting cost

	(1) OLS	(2) IV	
		First stage	
Dep. var.		Average hiring credit	Hiring cost
Share eligible hires		0.030***	−0.003
		(0.003)	(0.012)
Duration of vacant jobs		0.020*	0.851***
		(0.011)	(0.081)
Separation rate		0.015***	0.341***
		(0.001)	(0.007)
		Second stage	
Dep. var.		Employment growth	
Average hiring credit (a_1)	0.694***	1.04***	
	(0.127)	(0.282)	
Hiring cost (b_1)	−0.050***	−0.061***	
	(0.016)	(0.018)	
Controls	Yes	Yes	
No. of Observations	1,588	1,588	

Notes: This table presents the estimation of equation (A19). In the OLS and the second stage of the IV estimation, the dependent variable is the average growth rate of employment from 1 December 2008 to 30 November 2009 in each employment pool x sector unit, among firms with 6 to 10 full-time equivalent employees in the previous year. The independent variables are the average hiring credit per employee and the hiring cost as defined in equation (A19). Following the structural model, we control for the wage growth in small firms. To control for local shocks, we include as covariates, the employment growth in firms with 10 to 14 full-time equivalent employees in the previous year and the shift-share predictions of employment growth, defined as in the previous sections. We also add the average 2006–8 employment growth at the regional level, as above. We also control for key cell characteristics: the distribution of firms age in the cell, the average share of part-time workers, and the share of firms with sales below 2 million euros in the previous year. The average hiring credit per employee and the hiring cost are instrumented by the corresponding average values of the share of eligible hires, of the duration of job vacancies and of the job separation rate among firms with 6 to 10 full-time employees over 2006–8. Weights are used: for each employment pool x sector unit the weight equals total employment among firms with 6 to 10 full-time equivalent employees in the previous year. Robust standard errors in parentheses. *Significant at 10%, **Significant at 5%, ***Significant at 1%.
Source: Pôle emploi and DADS (Insee).

of job vacancies and of the job separation rate, as instruments for the hiring credit and the hiring cost in 2009. Note that the structural estimation of the labour demand is simplified by the fact that the identifying shock has no impact on wages and labour market tightness, as shown in Section 6.

The results of the regression of employment growth on labour cost and hiring costs are presented in Table 8. We show in Supplementary Appendix E that the coefficient of the average hiring credit, denoted a_1, in the regression of Table 8 is the inverse of the absolute value of the marginal revenue elasticity α. The IV estimate of a_1 then entails that the elasticity of the marginal revenue with respect to labour is about -0.96. We also show in the Supplementary Appendix that the coefficient of the hiring cost, denoted b_1, is the ratio of the vacancy cost in terms of wage c_V/w_{t-1} over the elasticity α. Consequently, Table 8 implies that the annual cost of a vacant job represents 5.8% of the annual wage. Since the average duration of a job vacancy is about 0.2 year, the hiring cost amounts to 1.2% of the annual wage, which is in line with the available empirical evidence.[40] Overall, these values imply that the micro elasticity of employment to a

40. Kramarz and Michaud (2010) estimate that hiring costs represent 2.8% of the wage bill in France. Flinn (2006) finds that the flow vacancy cost is 1.5% of the annual labour cost at the minimum wage in the U.S. For an overview on several countries, see Manning (2011) Table 2, p 983.

change in labour cost, which is about -1, is consistent with the difference-in-differences and the IV estimates presented above.[41]

7.3. *Cost per job created in different contexts*

Let us remind that temporary hiring credits are one-off schemes that provide hiring subsidies during specific periods, whereas permanent hiring credits provide them whatever the timing of hiring. In both cases, the subsidy is paid for a short period of time (usually one year) following the hiring. Neumark and Grijalva (2017) report that 99 of the 147 hiring credits recorded in the U.S. over the period 1970–2012 are permanent. There is also a great diversity of hiring credits in OECD countries (OECD, 2010). Accordingly, it is worth analysing in detail the consequences of different types of hiring credits in different contexts. We first consider the cost per job created when the hiring credit is either (non-expected, one-off) temporary or permanent. We analyse these credits when they are either targeted at a small subset of firms or accessible to all firms, and when wages are either rigid or flexible. A related issue concerns the feature of permanent hiring credits, which can be time invariant, but also countercyclical, *i.e.* provided when the economy slows down only. To deal with this issue, we use the equilibrium model to explicitly simulate a context where the government is committed to provide the credits when the economy slows down.

7.3.1. Permanent versus non-expected temporary hiring credits. In this section, we compare one-off non-expected temporary hiring credits lasting one year (like *zéro charges*) with time-invariant permanent hiring credits. Let us remind that the cost per job created by a temporary hiring credit, set out for one year, corresponds to the monthly cost necessary to create one supplementary job at the one-year time horizon, whereas the cost per job created by a permanent hiring credit, set out without any foreseen time limit, is the monthly cost necessary to create one supplementary job permanently, *i.e.* on an infinite time horizon. The costs per job created reported in Table 9, are computed using the elasticities of employment with respect to labour costs derived from the structural model in the neighbourhood of the steady state (see Supplementary Appendices F.1 and F.2). The steady state values of the model are computed over the 2005–8 pre-recession period used for the estimation of the structural parameters (see Supplementary Appendix F.3). We evaluate the cost per job created for different values of the product market power of firms. In line with available estimates (Bouis, 2008), the markup is set equal to 1.3 in the benchmark case, corresponding to the actual situation in 2008. In the lower panels of Table 9, we consider two alternative cases around this benchmark: zero markup, which yields the constrained efficient allocation when the wage is endogenous, and a markup higher than in the benchmark, equal to 1.6, which yields higher unemployment rate.

The importance of the duration of hiring credits

Table 9 shows that *zéro charges* was implemented in the most favourable situation: it was temporary, targeted at a small subset of firms, and it occurred in a context where wages were rigid. The cost per job created by a permanent hiring credit targeted at a small subset of firms in the same environment would have been four times larger. This result is an immediate consequence of the relation between the elasticity of employment with respect to a change in average labour cost per job induced by a change in the wage of all workers, ε, and the elasticity of employment with respect to a change in average labour cost per job induced by a temporary hiring credit,

41. This elasticity is defined by equation (F11) in Supplementary Appendix F.1.1.

TABLE 9
Cost per job created (in percentage of the wage) by hiring credits in different contexts

Hiring credit	To small subset of firms		To all firms	
	Temporary	Permanent	Temporary	Permanent
		Baseline mark-up: 1.3		
Exogenous wage	25.2	96.9	25.7	102.9
Wage posting	25.2	96.9	26.1	217.0
		High mark-up: 1.6		
Exogenous wage	25.7	99.1	27.5	107.2
Wage posting	25.7	99.1	28.9	161.8
		No mark-up		
Exogenous wage	24.8	95.5	25.0	99.8
Wage posting	24.8	95.5	25.1	272.2

Notes: The cost per job created is gross, and as such it does not account for the savings induced by job creation in terms of unemployment and other social benefits that would have been paid in the absence of the measure. It also ignores the remaining social contributions paid by employees on these additional jobs. Hiring credits which are targeted at "small subset of firms" entail no equilibrium effects. Hiring credits which are available to all firms entail equilibrium effects. Temporary hiring credits are provided for hirings occurring during a period of one year. Permanent hiring credits are time-invariant and provided for hirings occurring at any moment. The cost per job created is computed using the formula derived in Supplementary Appendices F1, F2, and F3. The annual discount factor, β, is set to 0.95. The estimation of the structural model yields: $\mu=0.45$, $\alpha=-0.964$ and the annual cost of a vacant job $c_v/w=0.056$. The separation rate, q, and the duration of job vacancies 1/m are computed from the DADS and Pole emploi data. We get q=0.5, 1/m=0.2 on average over the period 2005–8. The unemployment rate in the benchmark situation reported in the top panel of the table is computed from the French Labour Force Survey (Enquête emploi), which yields u=0.083. The share of employees that benefit from the hiring credit on 30 November 2009 in firms with positive take-up in 2009 is computed from the DADS and from the "hiring credit file". We get $\eta=0.26$. The costs per job created when the price mark-up is high, and when competition is perfect, are reported in the middle and the bottom panel respectively. These counterfactual environments are detailed in Supplementary Appendix F.3.

$\varepsilon_\sigma=\varepsilon/\eta$ where η stands for the share of employees that benefit from the hiring credit on 30 November 2009 in firms with positive take-up of *zéro charges*. In our setup, η is about 26%. When the hiring credit becomes permanent, η goes to one and the cost per job created by the hiring credit is increased by a factor of four.[42] Accordingly, hiring credits are more effective at creating jobs at a low cost when they apply on short periods of time.

Targeted versus economy-wide hiring credit

Table 9 shows that the cost per job created by hiring credits accessible to all firms is only slightly bigger than the cost per job created by hiring credits targeted at a small subset of firms when the wage is exogenous. This means that hikes in recruitment costs induced by the increase in the labour market tightness associated with economy-wide hiring credits have only a small impact on employment. This result is the consequence of small hiring costs as stressed above in Section 7.2.2.

The adjustment of wages

The comparison of cases with endogenous and exogenous wages shows that the costs per job created are very close in these two cases when the hiring credit is temporary. Note that they are even identical when the hiring credit is targeted at a small subset of firms. This result is a consequence of the assumption of decreasing marginal revenue. This implies that each firm optimally increases the number of hires when the hiring credit is implemented instead of increasing the hiring wage

42. The cost per job created of a permanent hiring credit is equal to that of permanent employment subsidy if the firms and the government face the same interest rate, which is assumed here.

as long as the labour market tightness remains unchanged.[43] In this context, an increase in the reservation wage of workers, induced by the hike in the labour market tightness, is the only channel through which the hiring credit exerts an impact on wages. Namely, in the period where the hiring credit is implemented, firms are induced to post more job vacancies, which pushes the labour market tightness up. The improvement in job finding raises the reservation wages and then the equilibrium wages. The size of this effect is smaller when the hiring credit is temporary rather than permanent, as discussed above. Indeed, in our setup, this mechanism has a small impact on employment creation when the hiring credit is temporary since it increases the cost per job created by about one point of percentage. The small impact of the reaction of wages relies in particular on the relative low elasticity of the job matching function with respect to the unemployment rate, which is estimated at 0.45 in our framework.

Nevertheless, the reaction of wages has a very strong impact when hiring credits are permanent: the cost per job created by an economy-wide permanent hiring credit is about 2 times higher than when wages are rigid.

The market power of firms

When firms have more power in the product market, the wage elasticity of employment is smaller in absolute value (α is greater in absolute value—see details in Supplementary Appendix F.3). This contributes to raising the cost per job created. The market power of firms has another effect that goes in the opposite direction. It increases unemployment, which decreases the hiring costs and exerts a downwards pressure on wages, and then reduces the cost per job created.

As shown by the lower panels of Table 9, the cost per job created increases with the market power of firms in all cases except when the hiring credit is permanent and wages are endogenous. It is clear that the market power of firms has a small impact on the cost per job created for relevant values of the markup when the wage is rigid or when the hiring credit is temporary. However, when the hiring credit is permanent and wages react, the market power of firms can have a significant impact on the cost per job created, which decreases by 100 percentage points when the markup goes from 1 to 1.6. In this case, the further away from the constrained efficient equilibrium, the more effective the hiring credit is to close the employment gap.

7.3.2. Countercyclical hiring credits. Our previous analysis compares (non-expected, one-off) temporary hiring credits with permanent time-invariant hiring credits. Since the cost of job creation is smaller in recession, because the labour market tightness is smaller when unemployment is higher, the returns of expenditure on hiring subsidies is larger in recession. This can justify a more intensive use of hiring subsidies in recessions.[44] To analyse countercyclical hiring credits, provided when the economy slows down only, we assume that the economy fluctuates according to a Markov process between two states: a high-productivity low-separation

43. See the discussion in Supplementary Appendix, equation (F32). This mechanism is the consequence of decreasing marginal productivity of labour. It also holds in a model with wage bargaining instead of wage posting. It has been highlighted by Stole and Zwiebel (1996) and examined further in a search and matching model by Cahuc *et al.* (2008).

44. In the context of our model, recessions are driven by productivity shocks and there is no specific source of inefficiency during recessions which could justify the use of hiring credits. Countercyclical markups could create such inefficiencies. While there is some evidence that markups are countercyclical (Rotemberg and Woodford, 1991; Chevalier and Scharfstein, 1996; Wilson and Reynolds, 2005; Ravn *et al.* 2006), there is no consensus of the empirical literature on this issue (Nekarda and Ramey, 2013 and the articles they cite provide evidence for acyclicality or procyclicality of markups).

TABLE 10
Cost per job created (in percentage of the average wage) by temporary, time-invariant and counter-cyclical hiring credits, along the business cycle

Hiring credit	To small subset of firms			To all firms		
	(1) Temporary	(2) Invariant	(3) Countercyclical	(4) Temporary	(5) Invariant	(6) Countercyclical
Exogenous wage	25.2	97.2	49.9	26.1	106.1	53.4
Wage posting	25.2	97.2	49.9	26.8	256.2	101.0

Notes: Hiring credits which are targeted at "small subset of firms" entail no equilibrium effects. Hiring credits which are available to all firms entail equilibrium effects. Temporary hiring credits are non-anticipated, one-off and provided for hirings occurring over a period of one year during recessions. Time-invariant hiring credits are provided at every moment. Countercyclical hiring credits are only provided in periods of recession. The price markup of firms is set to 1.3. The cost per job created is computed thanks to simulations of the two-state search and matching model, described in Supplementary Appendix F.4. Details about the calibration of the model are provided in Supplementary Appendix F.4.3. The cost per job created is gross.

state with 7% steady state unemployment rate and a low-productivity high-separation state with 11% steady state unemployment rate, consistent with the French economy over the last 40 years (see Supplementary Appendix F.4). Table 10 reports the cost per job created by temporary hiring credits lasting one year during a recession (in columns 1 and 4), by time-invariant permanent hiring credits (in columns 2 and 5) and by countercyclical hiring credits triggered during the whole recession periods which last 25 months on average in France (in columns 3 and 6).[45] Table 10 shows that the cost per job created by countercyclical hiring credits is much higher than that of one-off non-anticipated temporary hiring credits lasting one year only.

A first reason is that slowdowns last 25 months on average, implying that a larger share of employees benefit from the countercyclical credit than from the temporary hiring credit (which by definition benefits hires occurring over a one-year period only). This effect approximately doubles the cost of countercyclical hiring credits with respect to temporary hiring credits lasting one year. Obviously, it would be smaller if the countercyclical hiring credit applied on shorter periods.

A second reason is that, since labour is less expensive during slowdowns thanks to the countercyclical hiring credit, firms substitute jobs in slowdowns for jobs in expansions. The effect of this channel is quantitatively very small to the extent that it changes the timing of job creation without modifying significantly its impact on overall job creation when wages remain constant.[46]

The third reason comes from the upward pressure on wages exerted by the increase in hires induced by the countercyclical hiring credit in recessions. The last column of Table 10 shows that this last effect has a strong impact. When the hiring credit is economy-wide, this last effect approximately doubles the cost per job created when the wage is endogenous compared to the exogenous wage case. Then the countercyclical hiring credits is four times more costly than the one-off temporary hiring credits. Such a difference cannot be explained by different shares of subsidized hires over employment only, as the share of subsidized hires in countercyclical

45. The differences between the costs per job created by temporary/time-invariant hiring credits in Tables 9 and 10 are due to the fact that the baseline economies differ in both simulations sets.

46. The comparison of columns 1 and 3 (or 4 and 6, row 1) of Table 10 shows that the impact of this channel on the cost per job created is quantitatively small, as the cost per job created by the countercyclical hiring credit is about twice as high as that of the temporary credit. This factor almost maps the ratio of average recession length—25 months—to the shorter duration of the temporary credit, which lasts for 12 months. Consequently, the first reason, related to the share of infra-marginal employees in the firm, explains almost all the difference in the cost per job created between one-off non-expected hiring credits and countercyclical hiring credits when the wage is exogenous.

policies is twice the one in temporary credits.[47] Therefore, the cost per job created by economy wide countercyclical hiring credits remains high, even if it is significantly lower than that of time-invariant hiring credits.

All in all, these analysis suggest, in line with the findings of Kaas and Kircher (2015), that there is little room for economy-wide permanent hiring credits, either time-invariant or countercyclical, when wages can react.

8. CONCLUSION

This article shows that a hiring credit targeted at small firms and low wage workers did have a significant impact on employment in France during the 2008–9 recession. All in all, the hiring credit was very effective. It allowed the government to create jobs at zero net cost in a short period of time.

The estimation of a search and matching model shows, however, that the effectiveness of this hiring credit relied on very special circumstances: it was (one-off, non-anticipated) temporary, it was targeted at a small subset of firms and it was implemented in a context with high binding wage floors and high unemployment. Among all these elements that have favoured the effectiveness of the hiring credit, it appears that its temporary nature was key. The one-off non-anticipated temporary nature of the hiring credit allows the government to lower the cost of entrants but not that of incumbent workers with limited effects on wages which need time to adjust. This implies that hiring credits can be effective to boost job creation at a low cost if they are non-anticipated and implemented on short periods of time. The search and matching model suggests that this conclusion also holds true for economy-wide hiring credits and even when the minimum wage is not binding. Nevertheless, a counterpart of this positive conclusion is that hiring credits create jobs at very high costs when they are permanent, either time-invariant or countercyclical, especially when there are no high wage floors. This suggests that they should be avoided in such circumstances.

Future research should aim at clarifying the differential impact of hiring credits and wage subsidies on job separations. In this article, we found that job separations were unaffected by the one-off hiring credit implemented during the 2008–9 recession. Consistently, job destruction is considered exogenous in the model used to obtain structural estimates. However, in other settings, job destruction could be reduced by wage subsidies or permanent hiring credits. In that case, their costs per job created could be lowered compared with one-off, non-anticipated hiring credits.

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47. Moreover, in the economy-wide endogenous-wage case, assuming that slowdowns last 12 months—instead of 25 months—entails a cost per job created of 49.5% of the labour cost by countercyclical hiring credits, which is about twice as high as that of the one-off non expected temporary hiring credits of the same duration.

Supplementary Data

Supplementary data are available at *Review of Economic Studies* online.

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